Title: Effect of double diffusion processes in the deep ocean on the distribution and dynamics of particulate and dissolved matter: a case study in Tyrrhenian Sea
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My main comment on the second revised version concerned the lack of clear evidence of retention– aggregation process. To address this point, the authors have now included two new plots which make use of a larger number of CTD profiles to statistically show the influence of staircases on the Junge index and the AOU. In my view, this is a useful addition that provides clearer evidence of the retention– aggregation process. While the differences between the two groups of profiles (with and without staircases) in the decreasing slopes of the mean Junge index and AOU with depth are weak, they exist. This new figure provides more convincing arguments than the former versions of the manuscript, and I thank the authors for this improvement. Statistically speaking, more could probably be done, but as it is now, the manuscript has already gained in strength, and the arguments are more convincing to readers.

I don't have any further major comments regarding the core of the manuscript.

We thank the reviewer for their positive comments regarding the statistical analysis. We have also carefully considered their remarks on the validity and relevance of calculating the Richardson number.

However, this new version includes a point about the Richardson number (Ri) in response to a query from one of the other two reviewers. Using this Ri causes confusion and does not provide any useful information:

My first comment relates to the way Ri is calculated, which makes use of buoyancy frequency (from CTD) and shear (from LADCP) profiles, both smoothed over a large scale (48 m, l. 268). At this scale, it is unlikely that shear-driven favourable regions will be detected. This is because, at these scales, the shear is usually far too weak and that open-ocean instabilities far from boundaries occur at meter-scale. The only reason Ri is sometimes below the theoretical value of $1/4$ in this dataset is due to the very weakly stratified layers. Shear-driven turbulence is driven by shear, not by weak buoyancy!

From my experience, even with a 10-m scale LADCP shear and N2, it is difficult to associate Ri (say lower than $1/4$, or lower than some number such as 1 to account for the low vertical resolution of the LADCP shear profiles) with, for instance, enhanced levels of turbulent kinetic energy dissipation rates (i.e. measured turbulence levels) unless you sample very active, shear-driven turbulent regions. Thus, using 48-m scale resolution in an open-ocean staircase environment, a worse configuration, will only exhibit weakly stratified regions, which could be achieved using N2 only. Attempting to link Ri <1/4 and turbulent mixing is meaningless with this configuration (l. 487 - 497).

My second comment relates to the interpretation of Ri in the context of staircases. As mentioned, (l. 532-546) in the text and by previous studies, vertical shear (low Ri) tends to prevent the onset of salt– finger instability. At l. 495–497, because staircases have almost homogeneous layers that consequently drop Ri below $1/4$, the authors state that turbulent mixing (= shear-driven here) is 'concentrated in the homogeneous layers of the staircases'. The fact that homogeneous layers exist is not linked with a shear-driven process but with a convective salt-fingering process. Using a 48-m scale Ri cannot be used to infer the existence of enhanced shear-driven turbulence in those homogeneous layers.

l. 566-57: 'Below 1000 m, reduced stratification and shear create low Ri conditions, which appear to be important for both the formation and maintenance of thermohaline staircases, as evidenced by both float measurements and cruise data in the basin's central region'
So you mean that low Ri (= shear-driven turbulence, if you can ever diagnose it with this Ri) is helpful for the formation and maintenance of thermohaline staircases?! This contradicts what you wrote about previous studies at l. 540–546. Some clear explanations of the processes are needed.

We thank the reviewer for this important and relevant comment. We acknowledge the limitations of estimating the Richardson number (Ri) in this context. The relationship between low Ri values and increased mixing (or turbulence) is difficult to interpret with confidence in this case, particularly given the low vertical gradients and limited vertical resolution of our measurements.

Given these limitations, we have decided to keep only the Ri visualization at the section scale (Figure 3d), where the general trends remain consistent regardless of the smoothing applied, and we believe this information is still valuable. We have also removed the Brunt-Väisälä and Ri frequency plots from Figures 6 and 7. Brunt-Vaisala frequencies now appear in Figure B1.

We have revised the paragraph to clarify that we are not claiming that low Ri promotes thermohaline staircase formation; rather, we are stating that low stratification and low shear, both of which contribute to low Ri values, are typical of the depth range in which staircases are observed.

Third review of EGUsphere-2024-3436 REV#2

Effect of double diffusion processes in the deep ocean on the distribution and dynamics of particulate and dissolved matter: a case study in Tyrrhenian Sea by Durrieu de Madron et al.

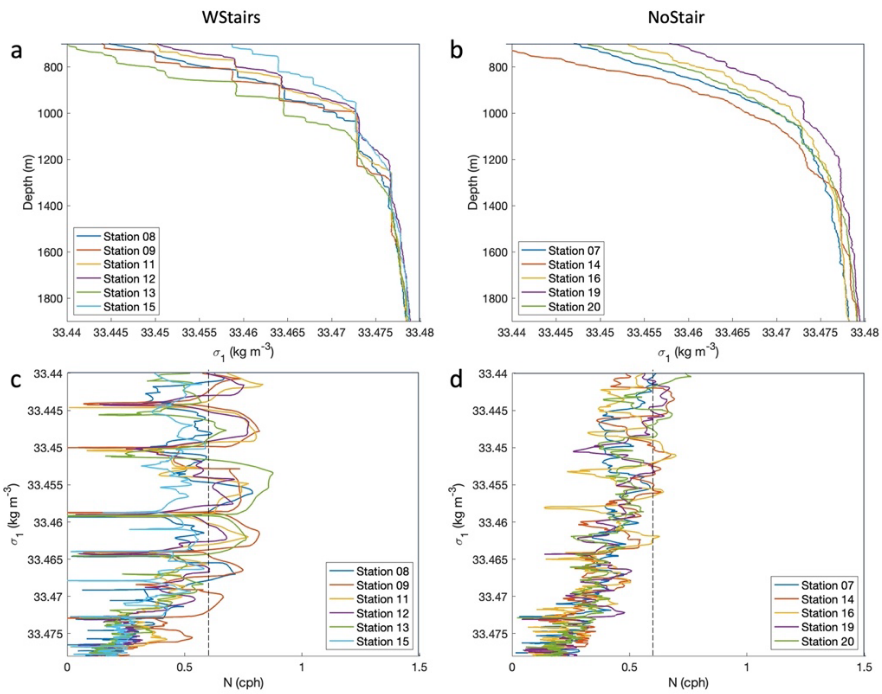
The authors have responded well to the reviewers' suggestions, and I believe the overall structure and readability of the paper, including the figures, are now excellent. The addition of stratification and stability parameters is useful, and classifying stations with and without staircases is an effective choice. There is a notable improvement in Figure 8.

Significant progress has been made in the statistical analysis, as wisely recommended by Reviewer #1. Despite this, I would still like to make a few comments:

We sincerely thank the reviewer for their insightful and constructive feedback.

Paragraph 2.3: The methods of threshold-based detection of stations could be expanded. It's clear how you selected the WStairs stations, which are six, but it's less clear why stations 4, 5, 6, 17, and 18 are excluded from the NoStair category. Why don't they fit your classification criteria? Splitting stations into two groups smooths out local variability, but it also assumes that the groups are comparable within each category. Heterogeneity within groups could affect the slopes and the significance, that's why you are not using all the 16 stations? This should be clarified.

We only used deep stations (i.e. those reaching a depth of 2000 m) for the categorization of stations (WStairs vs. NoStair) in order to obtain profiles of equivalent thicknesses. The stations not included are those at the edge of the basin with depths between 550 and 1500 m. To better illustrate the two groups, we have added the potential density anomaly and Brunt-Väisälä profiles for both groups in the appendix (Figure B1).



Line 422: There's a misalignment issue in Figure 5 with the numbers in the subplot.

Thank you for pointing this out. If the comment refers to the relative size of the first subplot compared to the others, this issue has been corrected. However, if the misalignment concerns the color bar values, please note that we were unable to adjust them, as the formatting is automatically managed by Ocean Data View (ODV) based on the number of decimal places, and cannot be manually modified.

Figure 9: When you mention shifted AOU profiles, do you mean both graphs start at 700 m, even though the linear regression for AOU begins at 1000 m? If so, the last two lines of the caption may be more confusing than helpful, as the graph is self-explanatory. Additionally, depth is plotted on the y-axis in all other figures and plots, including those for individual stations … except in this one. If this does not affect the readability of the plot from a statistical perspective, you might consider flipping the axis.

The AOU shift applied in Figure 9 concerns the AOU values, not the vertical position. This shift was introduced to better visualize and compare the oxygen evolution <u>within the staircase layer</u> across profiles. We have clarified this point in the figure caption to avoid confusion.

As for the axis orientation, we have now reversed the axes so that depth increases downward, consistent with the convention used in all other figures.

Line 612: You define WStairs and NoStair in 2.3, and paragraph 3.2.2 is about the positioning and persistence of the main staircases.

A reference to the WStairs group (profiles with multiple thermohaline staircases) has been added to clarify this ambiguity pointed out by your remark. The distinction between 'Wstairs' and 'NoStair' profiles lies primarily in the presence or absence of well-defined staircase structures in the 500–1000 m layer. The persistence described in section 3.2.2 refers specifically to the deep staircases observed below 1000 m. These clarifications have now been included in the revised text of section 2.3.

Line 614: If I am not mistaken, Aiken et al., 1991 refers to a book. Could you specify the exact location of this reference within your manuscript?

The reference specifically concerns Chapter 2 of the book: Interactions Between Continuous Predictors in Multiple Regression by Aiken, West, and Reno (1991). We have updated the reference in the bibliography to indicate the relevant chapter, rather than citing the entire book.

Lines 610–625: While the slopes are very close in value, you report a p-value indicating that the difference is statistically significant. But, given the small magnitude of these differences and the brief description of your statistical approach, this section could benefit from a more detailed explanation to clarify the reasoning behind the significance and its implications. For instance, you could include some details about the statistical approach in the Methods section (paragraph 2.3), which would help keep the discussion section more concise and focused without losing information.

While the slope differences are indeed small in magnitude, the statistical tests indicate that the effects are nonetheless detectable. This suggests that the presence of thermohaline staircases has a measurable, albeit moderate, impact on the vertical dynamics of particles, as inferred from parameters such as the Junge index and AOU.

In response to the reviewer's comment, we have added a more detailed description of the statistical methods used in the Materials and Methods section (paragraph 2.3). We have also slightly reworded the discussion text accordingly.

Third review of EGUsphere-2024-3436

Effect of double diffusion processes in the deep ocean on the distribution and dynamics of particulate and dissolved matter: a case study in Tyrrhenian Sea

by Durrieu de Madron et al.

Overview

The manuscript by Durrieu de Madron et al. presents CTD, ADCP and optical profile data along a section crossing the Tyrrhenian Sea and investigates salt finger staircase effects on particulate and dissolved matter distribution.

The authors have adequately addressed all the reviewers' comments which has strengthened the manuscript. I only have some minor further comments below.

We deeply thank the reviewer for its additional constructive comments and suggestions.

Line 308: stratification is lower

Corrected

Figure 3d: Ri would be best shown in log10 scale to emphasize Ri <0.25

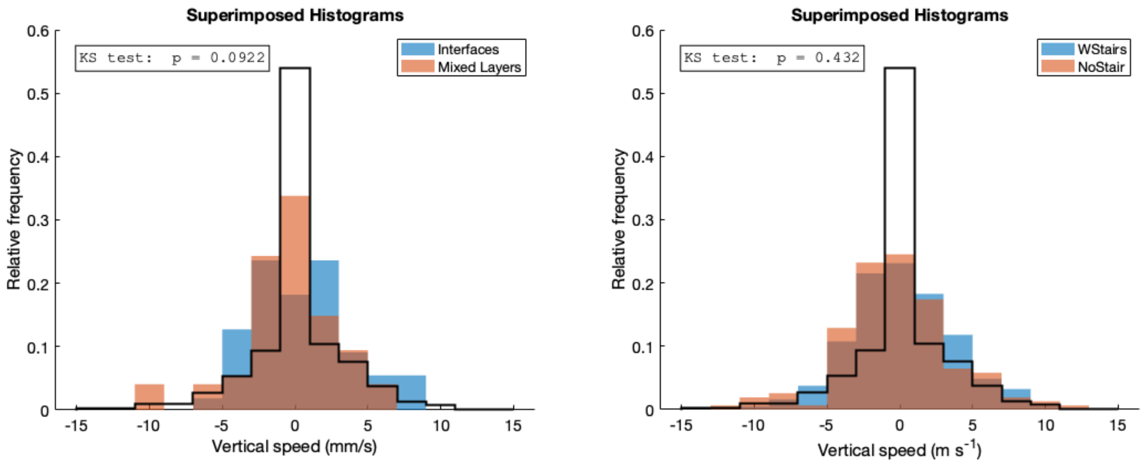
We implemented a log10 scale as suggested. However, considering the measurement uncertainties – also pointed out by Reviewer 1 – we chose to display only a few key isolines (not including the 0.25 threshold) in order to emphasize the clear contrast observed: below 600 m, Ri values systematically decrease with depth. The staircase zone consistently exhibits low Richardson numbers (Ri < 1), underlining its dynamic instability despite the limitations in resolution and precision.

Lines 320–329: The part about vertical velocity seems a bit half-baked, if you are showing the figure perhaps there can be a bit more to say? For example, is there a significant difference in the histogram for w within the steps vs outside of the steps? Or at least between profiles with vs without steps?

Thank you for this suggestion. To address this point more thoroughly, we extracted vertical velocity measurements between 700 and 2000 m depth, focusing specifically on values at the main density interfaces and within the mixed layers for all deep profiles. This depth range was chosen to target the zone where thermohaline staircases typically develop. Due to the limited vertical resolution of velocity measurements (40 m), the number of data points in each category remains modest (a few dozen).

We conducted two comparisons, each expressed in relative frequency (the black line corresponds to the entire dataset, including all stations and the full-water column): (1) vertical velocities within mixed layers vs. at interfaces, and (2) velocities from profiles with staircases ('Wstairs') vs. those without ('NoStair'). Since the Kolmogorov – Smirnov statistical test yielded p-values greater than 0.05, we conclude that the differences between the vertical velocity distributions (WStairs vs. NoStair, and Mixed Layers vs. Interfaces) are not statistically significant. The two populations can thus be considered similar in terms of distribution.

To reflect this more detailed analysis, we have replaced the original Figure 4 with a revised figure showing histograms of vertical velocities for the 'Wstairs' vs. 'NoStair' condition and for the entire dataset.



Line 322: in the upper 300 m

Corrected

Line 428: across the basin from west to east during

Corrected

Line 461: absent in the upper 1000 m at the stations within the anticyclonic eddy

Corrected

Line 473: delete 'drops'

Corrected

Line 613: particulate organic matter?

Indeed. Corrected

Line 653: vertical flux

Corrected

Line 667: vertical fluxes

Corrected